



Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at <http://about.jstor.org/participate-jstor/individuals/early-journal-content>.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

PSYCHOLOGICAL LITERATURE.

Proceedings of the English Society for Psychical Research. From July, 1882, to May, 1887.

Phantasms of the Living. By EDWARD GURNEY, M. A., FREDERICK W. H. MEYERS, M. A., and FRANK PODMORE. Two Vols. 1886.

The movement out of which the English Society for Psychical Research grew seems to have been Professor Barrett's paper before the British Association in 1876. This was the year in which the experiments of Mr. A. E. Outerbridge became known in Philadelphia, and in which Dr. George M. Beard, of New York, began his publications on muscle-reading. Unlike these American writers, Professor Barrett surmised that muscle-reading was an inadequate explanation of the "willing-game" just then becoming popular in England, especially when the transference of impressions was accomplished without contact, and the older theory of thought-reading seems on the whole to have prevailed. Although the problem termed by Professor Barrett "supersensuous perception," was the vital and chief experimental one, the English Society, at its organization in 1882, had in view the broader object of investigating modern spiritualism generally. The president, in his opening address, declared it "a scandal that disputes, as to the reality of these phenomena, should still be going on," claimed that there was at least a "*prima facie* case for investigation," and warned his hearers that in so strange a field it was only very gradually that all the complicated precautions needed to exclude possible illusion or deception could be learned. Professor Barrett, too, recognized, though not very graciously, Dr. Beard's six sources of errors, and referred to the prejudice with which this subject is usually approached.

Thus far not only the formation of such a society, but the boldness of its plan, with its committees on apparitions and haunted houses, and on the claims of Mesmer and Reichenbach, and the degree to which the difficulties and dangers of the proposed investigation were realized, were all such as to commend it, not only to every psychologist, but to every true and intelligent friend of culture and of religion. While those who regard the baser forms of modern spiritualism as the refined and concentrated embodiment of all the superstitions of a remote and barbaric past, and the claims of those who pretend to mediate between the living and their friends who are dead, as a nameless crime against the most sacred things of the soul, must feel a deep interest in such work, there is another class, perhaps still larger, and with an interest still deeper. This class consists of those who, in these days of unsettlement in religious beliefs, hope to find amidst superabundant *aberglaube* a nucleus of certainty for at least the doctrine of immortality. The most absolute idealists are not so satisfied with the speculative method which works by exhausting thought-possibilities as not to

welcome the most empirical refutation of materialism and mechanism. Even Mr. Meyers's "phantasmagoric efficacy," his "telepathic percolation," or veritable ghosts of those dying or dead, or even in great danger, are not unwarrantable in establishing his "solidarity of life which idealism proclaims," or "the universal mind in which all minds are one." But the impartiality attainable in most fields of scientific research, while it is the ideal to be striven for, in fact is impossible here. A rigorously unbiassed, and yet an intelligent jury, could probably not be found in this country, or in England, so many and subtle and remotely ancestral are the conscious, and far more the unconscious, prepossessions which enter like Schopenhauer's primacy of the will, making us all lynx-eyed to all that favors one side, and bat-eyed for all that favors the other. It is the fact of this inextinguishable bias (which, as has been well said, evolved from a state of savage superstition, so predisposes men that every occasion for it to show itself is utilized, and is therefore dangerous to modern culture and civilization, which enters unconsciously into our judgment regarding all such evidence as seems yet attainable in this field, weakening strong and reinforcing weak proof), that must be constantly kept in mind in seriously striving to form a just critical estimate of the voluminous printed work of the English Society. Even in reiterating the oft-expressed regret that so few men of science, trained in habits of exact observation, to offset the per-fervid and ever-fascinating exuberance of Mr. Meyers, who imposes on his brilliant imagination least of the temperance and suspense bred by the methods of modern science, cooperate in the work of the Society, and especially that trained psychologists and alienists should hold aloof from it, we shall seem to many to express a bias of our own. To these matters we shall recur.

Mr. Creery, a rural English clergyman, had five daughters, who were between the ages of ten and seventeen, in 1882, when the first report was made. Mr. Barrett, who is, we think, a professor of physics, certifies that they were "all thoroughly healthy, and as free as possible from morbid or hysterical symptoms." Their father states that from October, 1880, he had spent "night after night for several months, an hour or two each evening," in experiments on thought-transference with his girls, and seems to have noticed none of the hysterical symptoms, excessive fatigue, dizziness, trance-like inclination, faintness, *égare* look, or other pathological effects that alarmed other observers and correspondents of the society who tried similar tests with adolescents. At Easter, 1881, Mr. Barrett was admitted to the family seance. The most meagre account of the conditions of this session are given, and no record was kept of the number of guesses allowed. In August Mr. and Mrs. Sidgwick made a few tests, the conditions of which are not detailed. In November, and again in February of the next year, Prof. Balfour Stewart did the same, and reports his results, stating, however, that "while they cannot stand on the same footing as those of Professor Barrett" and others, they may have corroborative value. In April, 1882, Mr. Meyers and Mr. Gurney tested the girls in sessions covering six consecutive days. In August the three oldest girls went to Cambridge, where experiments were renewed day by day for ten days also by Mr. Meyers and Mr. Gurney, and in December, 1882, Professor Barrett again, alone, renewed tests in Dublin. The perceptive power of the children gradually declined during this time, "so that at the end of 1882 they could not do under the easiest con-

ditions what they could do under the most stringent in 1881." This decline, it is later stated in 1886, "had nothing whatever to do with any increased stringency in the precautions adopted."

That the precautions did grow more stringent there is abundant evidence. At first "*all* silently thought the name of the thing selected," after it had been written down and showed to the rest of the family. "The presence of the father seemed decidedly to increase the percentage of successes." At Cambridge, where the three elder sisters were isolated from their family, and where usually none of the sisters but the guesser knew the card selected, the successes were less numerous. Very significant is the series of thirty-two experiments where the sisters knew, and only the tops of their heads were visible to the guesser, and the suit was named correctly fourteen times running, with great positiveness and reiteration, while the card was right but five times. From twenty-seven experiments at Dublin, with the other sister knowing, the committee felt justified in saying that the presence and assistance of the sisters made no appreciable difference in the result, while at Cambridge only eight trials without and seven with the sisters knowing are given as the basis of a similar inference. From these random data the careful reader must infer that the effect of the presence of other members of the family was far from sufficiently studied. Indeed the opposite conclusion from that of the committee is suggested. The latter expressly ascribed failure under strange conditions to diffidence, and aimed mainly to exclude only *conscious*, and underestimated unconscious collusion, which the long previous practice at home must have made almost inevitable with a set of adolescent girls, however honest or healthy. We should even like a more explicit statement as to how the other sisters were excluded from knowledge of the object selected, where they were, etc.

The methods of isolating the guesser are perhaps still less satisfactory. At first (Easter, 1881) the child was *sent* into the next room, and the name of the object was written and showed around. In April, 1882, the child was recalled by one of the experimenters and movements in the room were excluded after the recall. Later a watcher was sent from the room with the child, and on being recalled the latter was placed with her eyes away from the company. Once at least she was isolated behind the door, at Dublin "behind an opaque curtain" [although we have found an hypnotically sensitized subject who could read large letters in sunlight through five thicknesses of cotton sheeting]. In this most vital respect also there was not only no rigorous control and no systematic method, but only the first rude and irregular preludes and approaches toward it. Such conditions also can serve only to satisfy those present, or with the bias for a pneumatic view of things above referred to.

Again we cannot forbear asking if *every* experiment, without exception, in Mr. Barrett's session of Easter, 1881, to say nothing of Mr. Stewart's sessions, was recorded; and if the results were all noted on the spot and the conditions and descriptions taken at the time and place. The experience of the Seybert commission in these respects, especially the latter, as well as the writer's own experience with the untrustworthiness of memory, even for an hour, where such complex conditions and interests are involved, are sufficient to justify this query. The report of July, 1882, is not explicit on these matters, and, from a careful scrutiny of it, it appears

at least doubtful. When was the paragraph in quotation marks beginning page 21 written, and when the very general description of conditions on page 20? If Mr. Barrett himself made or dictated these notes on the spot, how could he, as the only person present besides the family, possibly so secure himself against all the manifold and subtle sources of fallacy in observation as to be warranted in calling his tests on this occasion "absolutely unexceptionable and conclusive," as he recklessly does. Next in importance to the method of experimenting in so hazardous a field is the way of making the protocol and working up the final form of the report. Either this or the accuracy of observation, or more probably with but one observer, however good, against so many possibilities of error, both must suffer. If either of these surmises is correct, it bears against the statement that the girls gradually lost their power from any cause but increased precautions on the part of the experimenters.

In some of the Creery experiments the precautions of the committee might possibly be impaired by the number-habit, then unknown, and now probably but partially explored, or by corneal reading, or quite broken through by muscle reading without contact of the manifold, and as yet but little known, forms and possibilities which the committee seem to have mainly ignored throughout. We know as yet comparatively little about the constitution and laws of what Dr. Carpenter used to call our automaton. One thing however, is more and more realized, viz.: that it is far more sensitive than our super-liminal conscious sensation, that it is indefinitely more complex and manifold, that no culture has ever approached in its diversity, subtilty or unity the limits of brain possibility in these respects. Neural is far wider than psychic action—the latter involving the former, but not the former the latter. Only when the former reaches a certain intensity, and perhaps extensity, and lasts a certain time, does consciousness, which represents as through a loophole aggregate cerebral states and changes, arise. In the study of hyperæsthetic states we now begin to realize the possibilities of our sensory organism, and how greatly the limits of just observability vary at different times and states and in different persons, and how it responds physically to the remotest and faintest cosmic influences. Hearing, *e. g.*, which is known to vary exceedingly among people whose auditory sensitiveness passes for normal, the writer has carefully tested in many persons. Two individuals were selected for exceptional acuteness in this sense, and the following simple code devised by the writer, which, though repeatedly tried in critical companies, has never been detected, and with results that impressed many as genuinely telepathic. Pulsations, felt subjectively by the percipient, and easily counted by the agent, either by movement of the toe if one leg was crossed on the other, or directly seen in the aorta, or in vibrations of hairs or neck-ribbons, were the basis. The faintest respiratory noises through the nose, or even mouth, of the agent, were made to coincide with and accent certain pulsations of the percipient. To make these modified breathings so distinct as to be clearly heard at a distance by the normal sensitive, yet so faint as to be inaudible even if listened for by spectators often nearer than the agent, was the art of the latter. If a number was selected by some one present the agent caught the rythm of the percipient's pulse, and could hold it for some time if blindfolded, or then see it in the toe occasionally,

and gave a very faint sniff coinciding with a pulsation, and from this, as zero, the percipient counted till the next sniff for the first digit, then till the next sniff from the second digit, and so on. For cards, first the suit and then the card, were counted off. The alphabet went more slowly, but a series of diagrams, from which the selection by the company was made, which had been memorized and numbered, was the greatest success. When the percipient's ears were stopped, coughs, jars on the table or floor, etc., helped us out. This trivial code, however, essentially depended ultimately for the absolute security it generally possessed on the fact that the percipient could hear more acutely than any one present, and when that is the case a telepathy not outside the ordinary channels of sense is possible. We have no data whatever for believing that the ear can hear and distinguish muscle or pencil sounds in making different letters, hear whispers through a couple of doors, etc., but neither have we adequate data for judging how far these, or even less desperate possibilities in the field of vision, would need to be stretched to account for some of the Creery phenomena. Again, a man enters a strange house and wills with the family that a girl in another room bring him certain things in various parts of the house. Would he not almost inevitably in the first session, follow in selecting objects, the suggestions of those who knew what was in the house, and whose mind had been trained both generally from infancy and specifically by the long-continued practices with each other, to concilient action with that of the percipient, and who would moreover not wish her to fail in such an exhibition of her power? When saucer was suggested and written, and plate was brought, the percipient said, saucer "came into my head, but I hesitated, as I thought it unlikely you would name saucer after cup as being too easy," indicating that here, at least, she was controlled by deliberate judgment about the degree of ease or difficulty with which a test was suggested by the preceding one. This suggests clever prediction of the action of minds whose processes are thoroughly familiar—a kind of integrated as opposed to multiplex personality, as in the case of the Siamese and less identified twins—at least so far as the lapse of ordinary associative thought is concerned.

Our chief regret after all is that in the Creery tests the methods were so variable and the results so inadequately worked up and presented, that those whose interest does not centre in the factitious problem of telepathy can get so little aid from them. The poise of a Newton would have even withheld all results till at least some mode or law was either demonstrated or at least disposed of. Is it color or form, suit or card, eye or ear or muscle-sense, that is the preferred medium of transfer? These vital points, towards which we think a truly scientific mind would instinctively tend, are only cursorily mentioned and apparently as afterthoughts. They could best be studied by a careful scrutiny and tabulation of errors which would very likely prove more luminous than successes, and these were not only not printed, but appear to have been only recorded as errors. The color of the card, *e. g.*, ought to be right twice as often as the suit. Were it still more often right it would suggest that transference could be more hopefully sought in the field of color; if one color was chosen most often, that preference would be a fruitful theme for further research, and suggest the kind of precautions, etc. If nine and five are often confounded it would suggest transfer by ear, as in the case of a fleshy and wheezy

agent we heard of whose unconscious laryngial innervation, while thinking of figures and numbers, suggested them to a percipient; if three and two, it might suggest muscle-sense, as the tops of both figures where the hand begins to make them, are alike, so that they are often confounded in muscle-reading, as also are six and eight. If the letters most often mistaken are those shown by Javal's tests to be those with least individuality and most liable to be confused at a distance, in sunlight, etc., we should infer visual transfer. For all such directions of studies, however, or for such data as the experiments made must have afforded had the record been complete and full, we seek in vain. Thus if we conclude, from the very striking results presented, something unknown and independent of consciousness and will, the alternative is by no means necessarily something extra-sensuous or immaterial, or in any degree absolved from the conditions of time and space. So far as this is inferred, we have only another illustration of the inveterate vulgar tendency to associate all unusual manifestations of man's unconscious automatic nature with supernatural powers, whether of good or evil. No special study of such popular chapters of psychology as dreams, witchcraft, hallucination or hypnotism, unless made on the basis of long apprenticeship in experimental biology and physiology and the study and observations of nervous diseases generally, is likely, as it seems to us, to give one that sense of the depth and breadth and number and subtlety of physical processes underlying and overreaching and encompassing our conscious psychic activities that is so indispensable just at this point, to prevent an investigation so well begun and representing such high attainment and ability from ending—we will not say as abortively as the other well-known attempts to explain these phenomena made within a century, for it is already far beyond them in methods and results, but without utilizing all the rays of light which modern science now sheds from so many and widely separated points upon the great central questions of psychology. The percipient's descriptions of his mental processes so often adverted to in the reports of the Society are probably of less value than the long account of their symptoms neurotic patients are wont to give physicians who will listen from courtesy and diagnose quite independently of the patient's morbid legend. The degree of reliance on the undoubted good character and *mens conscia sibi recti* of their subjects the committee manifest, despite the irrelevancy of these considerations, which Dr. Geo. Beard has made so plain, and perhaps even the uncalled for allusion to their own veracity and intended accuracy ["The possibility of collusion was excluded unless our own veracity be impeached,"] which are beyond all question, are by no means designed to give hostile criticism an air and sense of personal discourtesy, but are only an irrelevancy expressing the tendency we deprecate.

We have dwelt with some detail on the Creery phenomena because Mr. Gurney, Mr. Meyers and Professor Barrett, the most active workers and probably the best observers in the English Society, spent so much time on them, and because the former expressly states that "it is to those trials that we owe our own convictions of the possibility of genuine thought-transference." Hence a bias certainly existed in all subsequent experiments. The precautions grew more strict, and probably, as we infer, the record grew fuller and more exact, and what is called a decline in telepathic

power we should interpret from between the lines of the record as an approach to the heart of the mystery, which ought to encourage unbiassed investigators to press on toward a beckoning goal. The girls grew discouraged and did not succeed with each other, it is said. This is natural, as interest in their performances diminished. But it is strange that this decline should coincide step by step with closer study of them, and still more so that all the girls should lose this marvelous power *simultaneously*! Never was a momentous discovery wrested from nature with less labor. Groups of agreeable ladies and gentlemen at play sustain each other's flagging interest by the admonition that the sensuous demonstration of the reality of the world of things spiritual and immortal is at hand. "One should not let one's self be discouraged," says Herr Schmoll in the proceedings for May, 1887, "by a little trouble when there is a chance of throwing light on events which, correctly apprehended, may lead us to the psychological proof of our transcendental, imperishable ego." That these investigations have struck the trail of something new and strange, however rare and abnormal it may be, there is ample evidence; but so far they have given us only the opinions of individuals either emboldened or perhaps formed by very exceptional experience in a field of great popular interest and little positive knowledge, attractively narrated some time afterward with illustrative extracts and a few very summary tables from notes taken at the time.

The second group of evidence for thought transference, next in importance to that obtained from the Creerys is that where forms, often not easily described in words, seemed extra-sensuously transmitted. In the first series of these Mr. Blackburn, an editor, was the agent, and Mr. Smith, "a young mesmerist living at Brighton," was the percipient. They had frequently practiced together previous to their first meeting with the committee in December, 1882. The method was as follows: Mr. Smith was blindfolded and seated with his back to the committee, but in the same room with them. Figures were drawn by the latter, and after Mr. Blackburn had looked at them, and then held Smith's hand awhile, he released it, when Smith drew the figure, remaining blindfolded while drawing, as expressly stated in italics. We beg our readers who may be sufficiently interested to try and draw closed lines, bringing the end to exactly the beginning, as is done in fourteen or more cases in these reproductions, with their eyes closed. We have repeatedly tried and failed with each of these forms, whether drawing slowly and irregularly, as in the earlier figures, or smoothly, as in No. 7. Hence we infer either that Smith saw while drawing, which seemed to the committee as likely, and no more so than that he saw while divining the figure, or else that we have here to do with another mild marvel to which they have not called our attention.

About six weeks later experiments for three or four consecutive days were conducted at the rooms of the society in London with the same subjects, and still another series in April, 1883. In all thirty-seven figures were drawn for reproduction, of which fac similes of twenty-two are published. Of these five were with contact. These, however, and even the first series, we may disregard, as it now became with the committee a "cardinal axiom on this subject that no experiment where contact of any sort is allowed can be decisive." For the remaining seventeen the conditions seem more strict than for any tests yet made. The agent was seated and watched contin-

ually in one room, and the drawings were made in another which the committee did not leave. After seeing the picture, Mr. Blackburn, with closed eyes, was led into the room with his sensitive and placed behind him at a distance of some two feet. The percipient sat and drew with or without bandage as he chose, and the reproductions were at once secured. With the record of these seventeen reproductions, without contact, of the most unconventional diagrams we confess ourselves more deeply impressed than with any other work of the society, especially the remarkable No. 22, reproduced with the ears of the percipient stopped with putty, and wraps enveloping the entire upper part of his body. We can but wish, however, there had been more of these, and consecutively, and that while they were about it the committee had isolated the lower part of Mr. Smith's body from all sensation of jars, and carried, rolled or swung the agent into the room, to exclude the possibility of a code by steps which an American exhibitor has brought to an incredible degree of development, and also tested the amount of reduction of acuteness of audition in each ear of Mr. Smith, or at least of themselves, by putty, and taken precautions to make sure that all light from the floor was excluded from Mr. Smith's eyes. We have practiced with some success a toe-code, a part of which is by minimal movements of the great toe within a thin shoe, the latter not moving at all, and a part, for complex figures, involving slight movement of the toe of the shoe, which we should think would only be facilitated by the conditions of No. 22 with overhanging wraps to shield it from the committee. Moreover, it is just these larger general forms and relations of parts without finer details that are best transmitted thus, while the latter, as the committee note, are absent. It seems worse than Mephistophelean—indeed we wish we were freer from the fear that it is so in very truth—to even suggest, in place of tension toward transcendental entities, slight practiced movements of the big toe. We do believe, however, that the number of possible keys and codes by which these things can be done is far greater than the committee seem to realize, and even that very subtle forms of deceit are sometimes automatic and quite unconscious in the most worthy people. The first four figures where contact was allowed are certainly much better reproduced than any other four in the series, and in Figure 13 an entire change of the percipient's conception of the model was caused by contact, though unfortunately it is not stated whether Blackburn touched Smith again after he had corrected his conception of the original, and before it was correctly reproduced, or whether the second reproduction coincided with Blackburn's memory of it. Again, in Figure 6, contact suggested a remote reproduction. Suggestive, too, is the circumstance that after contact was excluded Mr. Blackburn explains so many deviations in the copy into greater conformity with the originals by mistakes in his own conception, which he had done in no previous case in this or the earlier series. A cross in one would grow too large in his mind's eye against his will; he had "not precisely remembered" another; forgot the eyes in another; focused on one part only in another, and imagined curves in the opposite direction in another. The tests to account for the mental inversion of objects, which strikes us as just the thing, were only forty-two in number, the result being that eighty-seven per cent. of the answers gave the direction in which a vertical and only thirty-seven per cent. gave that in which

a horizontal arrow pointed. May we add that we have found the same advantage of perpendiculars in the toe-code, on account of the relative difficulty of lateral movement? This is doubtless entirely irrelevant, for it is expressly stated that Smith sees in his mental shrine the image of a white arrow on a dark ground and instantly detected the change when a white arrow on a red ground was substituted for an ink-drawn one. This aside, however, we deem the evidence considerable that after contact was given up either a new and less practiced or more difficult code (conscious or unconscious) was used, or else that the unknown telepathic forces were obliged to find and deepen other lines of least resistance.

Mr. Malcolm Guthrie, well known as a writer on the Spencerian philosophy, is the centre of another important group of thought-transferers, and the percipients are young lady clerks in his drapery establishment in Liverpool. His first tests had been made upon his son. "a nervous and susceptible fair-haired boy of ten," who was at first very successful, but whose efforts made him "feel queer," and who was soon "disposed to ensure success by taking a sly peep at the object." Here he would have stopped had he not learned of the success of the lady clerks, who had practiced by themselves, stimulated by an exhibition of Irving Bishop. The protocol here is admirable, taken on the spot by Mr. Birchall and printed in full, and Mr. Guthrie is very positive in stating that there were a large number of "complete successes" where "the possibility of indication was excluded." The first session of April 4, 1883, consisted of four tests with contact and blindfolded percipient, and one pretty complete success. At the next session, which was without contact, the ladies alone were present. In the frequent sessions in April and May and the following fall, thirteen in number, with from one to over a score of tests each, Mr. Guthrie and Mr. Birchall were generally present with the ladies. The party sat in a semi-circle facing the percipient, and one, or more commonly all present, acted as agents, gazing at an object placed or held in front of them, but back of the blindfolded percipient's head. The first thing that strikes the critical reader is that failures are put down only as such, whether entire sessions as those of May 25, August 30, September 26, or single tests, while the words of the percipient and the conversation with the agent, showing the approximations to correct guesses, are quite fully given in successes, although here again failures would be probably more instructive, or at least as much so, as successes. The tests were mostly visual, and thus, so far from the process being "independent of the recognized channels of sense," as telepathic processes are defined, they are distinctly in the field of vision. Hence, if the vision was not due to normal retinal stimulus, however subtle in degree, the images must be centrally initiated and projected centrifugally outward and downward, which even Mr. Gurney is bold enough to urge, in the face of a strong consensus of neural specialists only for those of the more elaborated and variable sort. The percipient first "sees" light on dark, and next most frequently yellow, the brightest colors, and very general indications of form follow. The first attempt made to study the effect of colors might, if systematized and carried out, have told us whether the ease of perception followed the law of saturation or intensity, but was so badly arranged as to show nothing. This and much else makes us wish to know how the percipients were blindfolded, but we are

only told once for all that it was "effectually." How many thicknesses, of what kind of material and of what colors; how, if at all, they were tested as to their sensitiveness to light, which may be completely absent at first, but slowly regained in wonderful degree, as experiments show, by rest. We are not told the position of the light, or whether there was one or more, nor whether there were polished surfaces capable of reflection, whether access of light from below was permitted. The writer knows a young man who has given attention to the position and use of tiny mirrors, drawn by hairs or invisible threads from the shoe-sole, pants, etc., to enable him to see below a blindfold what was taking place above and back of him, trying even watch-guards and chains, bright buttons and eye-glasses carelessly hanging from his neck. In such tricks ladies might possibly receive even unconscious intimations from reflecting surfaces of stones in their brooches or rings, or indeed any polished surface capable of sending light under a handkerchief about the eyes. There is little indication that Mr. Guthrie is aware of the number and subtlety of the sources of error in such experiments as he conducts. We have ourselves conveyed indications of form to a confederate by slight conscious movements of the eyes. This code is a dangerous one, for the attention of all is so prone to fasten on the eyes, but the law of the dominance of contours and the motor elements of perception shows how unconscious and instinctive it is. With contact we have conveyed form by motions so slight as to tax even Goldscheider's limits of extensive sensibility by a grain of sand glued to the finger-tip, and deliberately drawn a figure on the dermal surface of the percipient by motions so slight as to escape detection. A very simple and rapid pressure code for figures, with almost no lateral motion, is worked with a little practice. Another possibility of error lies in the tests with names and letters. If one thinks of a letter and either says or points to each letter in the alphabet, a good muscle-reader divines what letter is in mind by unconscious and unavoidable modifications of finger or voice when it is reached. In the Guthrie tests a free conversation is held. "Has the word come to you?" says the agent to the hesitating percipient, who responds *z*, which is the last phonetic sound it "has." "Right," says the agent; "*i*" at once says the percipient. After guessing *o* rightly and hearing the word right again, now perhaps without any modification of any of its phonic elements, the percipient murmurs *p* and *m*, and when the agent says *No*, at once responds *n*, which is right. As there is internal evidence by indirect quotations and other ways that we do not have a full stenographic record of all the often protracted, conversation leading up to the correct guesser, suggestions unconsciously given and received of this kind are at least not consciously excluded.

In the report of November, 1883, sixteen reproduced drawings selected from one hundred and fifty obtained by Mr. Guthrie and his subjects are published. The original diagrams were "for the most part" made in another room and placed behind the agent, and later in those published, on the agent's side of a wooden stand on a table between him and the percipient, the latter being blindfolded. When the percipient professed herself ready to draw, the picture was concealed and the blindfold removed. Of the sixteen, which seem to have been produced after considerable practice, and with these more strict conditions, contact occurred

in but three cases, which are reproduced better than the rest, certainly if we exclude the first six complete and consecutive tests of a single sitting. With one hundred and fifty ever so partial successes it would seem that some induction could be made as to the parts of the figure or the forms that were best or worst reproduced. In those printed angles are nearly always retraced by the percipient. The same is true of curves, especially circles, with which angles are never confounded, although curves are repeatedly given in the wrong directions. It would seem that vertical were quite well distinguished from oblique or horizontal lines. A straight vertical line is drawn with a crescendo and then a diminuendo of rate, as Vierordt's experiments show, and also of pressure and of noise, as experiments with a spring pen on a large rotating surface made in the psycho-physic rooms of this University showed. Such a line is readily distinguished from a curve by the ear alone. A gradual change of direction involving new sets of muscles, a single movement in one direction, a sudden change in direction, series of repeated movements, large and small lines of the same sort, heavy and light, etc., are not hard to distinguish. A few tests with such a code, as near to nature as we could make it, have at least convinced us of possibilities, and we commend it as a subject for further special research as a kind of psycho-physic auscultation. This does not explain to our mind by any means all in all of the sixteen reproductions of this series, but we should like to know how these exceeding broad and scratchy lines of the originals, which are reproduced indifferently, now in very heavy and now in fine lines, were made. Such a suggestion, however, may after all only serve to divert attention from some entirely different mode of transmission.

In July, 1885, Mr. Guthrie reports further researches, assisted by Professors Lodge and Herdman, but complains of a falling off in the success of his experiments, shown also in his tables. One percipient had been lost, the novelty and vivacity of their seances he said was gone, and he had lost power to give off impressions. Whether the percipient had lost power he does not know. The professors do not appear to have made the precautions much more effective, although they placed the percipient blindfolded facing a corner, and placed objects on a screen at the back of her chair which were seen from behind by the agents. We are not told the position or number of the lights, the nature of the screen, the reflecting quality of walls or floor, what precautions were taken in placing and removing objects. Suppose the screen to be metallic or resonant, or even hard, or the objects handled without care not to hit them against things in a way to produce noise, then we may have suggestions by sound, as in the well-known game of guessing any one of a dozen objects by their sound when struck, which a well-known philologist thinks the primal origin of names of objects. Were precautions taken that no floor shadows of the object should be cast, and has Mr. Guthrie ever tried that other parlor game, once very popular in this country, of holding up objects at a distance of from a few feet to a few inches (according to the sensitiveness of its agent), from the face and neck, to be guessed by their differences of radiant heat? Surprising facility in this latter game we have known to be gradually lost by fatigue or consciousness, as with these subjects. In the ease with which colors are divined, especially if bright, the repeated substitution, in objects not well

known, of contours for surfaces and of surfaces for solids, all suggests, as one of Mr. Guthrie's subjects insists, real vision, and not a mental impression by thought alone. This circumstance, and the continued phenomenon of inversion of right and left, and of reminiscence, or late effect of a previous figure, seem to us very suggestively to invite further special research in each direction, which was not attempted.

Professor Lodge makes an independent report on his "some dozen sittings" with Mr. Guthrie's subjects. Like several physicists with whom we have conversed on this subject he conceives the relation of mind to brain as very likely analogous to that of electric energy to the conductor, as not confined to its contour, but exerting an influence "like a faint echo" in adjacent space, and so perhaps affecting other near brains, but so slightly that they do not commonly notice it. He says that no reliance was placed on or care taken in the bandaging, but he shows, although in a strange field, the training of a man of experimental science by the valuable suggestions of two agents, thinking at the same time of a different object, and again of two percipients and one agent, but neither was fairly tried.

Of the tests made by J. W. and Kate Smith, and by Max Dessoir, both, but especially the latter, are not only poorly reported, but seem to have been made with so inadequate a conception of the sources of error, that, although we are assured that "deception conscious or unconscious is altogether out of the question," the indications are, to our thinking, quite otherwise, and their reports of their accounts do not merit detailed criticism.

The tests made by A. Schmoll, translated in the proceedings of May, 1887, are decidedly more striking, but the eyes of the percipient were very slightly covered, merely, it is said, to make direct vision impossible; real objects were handled, and figures drawn with a match dipped in ink in the room, the time required to divine the object was very long, often fifteen minutes or more; the original drawings were not preserved; it was not even noticed at the time whether a watch, laid on the table to be seen by the agents and divined by the percipient, was going at the time or not (the notes stating that the ticking would be drowned by the noise of carriages in the street, was too far off, etc., but Mr. Meyers states that M. Schmoll had proved afterwards that it was not going at the time); all present generally acted as agents, so that no one was left to observe the percipient. The jar of heavy carriages referred to, while it would obscure sounds, might rattle some of the objects lying on the table, and thus suggest, by audition at least the tea-pot. Of the twenty-six experiments reported, some must be called complete failures, and it is a matter of individual judgment to say how many approach precision, which the experimenter claims for but eight.

The above experiments of visual form and hearing are, as Mr. Gurney says, by far the most important and convenient. The tests with tastes and smells, the latter of which is practically almost inseparable from the former in the case of many substances, were usually with contact; but even where the substances were kept bottled in another room and the hand of the agent applied to the percipient through a sliding trap in the wall, we are not even told by whom or just how the substance was applied to the sensory surface of the agent. The experiments of Vintschgau and of Camerer, to

say nothing of Jäger, show such subtlety of smell with flavors and aromatics that we need hardly assign more than great sensitiveness, hardly amounting to hyperosmia or hypergeusia, to account for all that is reported in the field of these senses. Pains again are so closely associated, by such subtle reflexes, with motor reactions or tendencies to the same, as was experimentally shown in the well-known demonstrations on the reflex frog in Ludwig's laboratory by Baxt, and, as Mr. Gurney pertinently adds, are readily applicable only to a few widely separated tracts of dermal surfaces, that muscular suggestion is almost inevitable, and we think by no means excluded in any of these tests.

We have thus very hastily reviewed all the leading experimental work of the society. Mr. Gurney states that "from an evidential point of view" the facts are "of an extremely simple kind," and Dr. Morton Prince, of Boston, gravely says that "no physical experiments in the laboratory have been more under the control of the chemist and the physiologist than have these." The simple conditions of experiment are, it is said, to exclude unconscious guidances and contact. The exact opposite is true. The conditions are as infinitely complicated as the psycho-physic constitution of man, and the sources of error are as much more numerous than those in physical science as man is more complex than the substances and forces it studies. What individual can catalogue all the scattered known sources of error, to say nothing of those as yet unknown, in this vast field? Fallacies of observation, of evidence, of language and statement, defects of character and heredity, tricks of our automatic nature, subtle and manifold far beyond all conception, the countless possibilities of illusion conscious and unconscious, so great as to suggest that the boast of the great French magician that he would agree to make any man believe in the normal state that he saw anything, may not have been so very wild; the unfathomable passion for deceit, both conscious and unconscious, that sometimes runs in veins through the natures of men of best reputation and most honest purpose—all these and many more are involved and must be exhausted before telepathy can be positively demonstrated as a residual fact. Hyperaesthesia, too whether normal or morbid, opens up a new world as truly as the microscope or telephone. One tells the form, substance and even color of objects near him by radiant heat, or reads as in a mirror, shadows from walls that seem to others unreflecting; or, in one lately reported case, sees the shadows of heat vibrations over a hot substance cast on a wall by moonlight; the sense of a personal presence is felt by the blow or noise of breathing or heat. The case of Dr. Taguet's patient, who, it was said, was able to read script held behind her head by reflection on a plain card in front of her; the case reported by Dr. Sauvaire, who recognized the suit and number of a card in a different pack, as, it would *seem*, by seeing through it; the case of Rocha's clerk, who seemed to use a piece of card-board as a mirror in which he could see all that took place behind his back; and the well-known case of Bergson's reader of images reflected in the cornea—all these cases are very inadequately considered by Mr. Meyers. If these degrees of hyperaesthesia, normal or even hypnotic, are possible, and were possessed by the subjects with which the English society experimented, the entire experimental basis of telepathy vanishes. Moreover, there is a wide field of unexplored possibility. If blinded bats avoid objects in flying by fine sense of greater barometric pressure

near objects, we may reflect on the possibilities of perception of aerial pressure by highly sensitized subjects. We have no less good reason to complain of the very inadequate way in which the society has treated the subject of suggestion. We regard the book on this subject by Dr. Ochorowicz as one of the most valuable contributions in this field, as the best statement of the chief rival hypothesis of telepathy, and the one we think every truly scientific man must prefer so far; but the treatment of its contents by Mr. Podmore is very light, illustrating again, in fact, the easy way of ignoring serious difficulties and objections which characterizes the society. Then there are codes and signals, and sometimes quite elaborated languages, by steps, inflections, accents, intervals, rustles and movements of every mobile organ. Thus, not only by the arts of diverting the attention, which, if it is sharpened in one direction, is dulled in all others, but even in the very focus of attention the man of sharper can do what he will to and with the man of duller sense, and seem to work by forces "independent of the recognized channels of sense." Add now the extreme rarity of all those qualities of mind which make a good observer, and the strangeness and perhaps great rarity of the phenomenon, and the probability of error in so hasty conclusions is vast.

Dr. Prince states, as is often implied in the reports, that "no established law is controverted" by the conclusion of telepathy. But the law of "isolated conductivity" formulated fully by Johannes Müller, which Helmholtz compares in importance to the law of gravity, first brought order into the field of neurology by insisting that impressions never jump from one fibre to another. If the law be true, an optical impression of the highest intensity may pass along centrepetal retinal fibres within less than a hundredth of a millimetre of an auditory fibre without in the least being able to affect the latter. This law is so generally accepted as fundamental that Gudden states that "in the presence of an anatomical fact, all physiological facts that seem against it lose their significance." Indeed, two severed ends of a fibre cannot be put into so close contact that physiological action can pass from one to another unimpeded. Even those physiologists who admit that certain phenomenon may possibly be explained by the old theory of "sympathies," caused by a stimulus jumping across from one fibre to the next, admit that it is both rare and morbid. The oft-trusted illustration of magnetic induction certainly is not valid here. Is it likely that a neural state should jump from one brain to another, through a great interval, when intense stimuli on one nerve cannot affect another in the closest contact with it. An American essayist at a scientific club lately claimed that all associative processes, by which one idea or impression calls up the sequent state of consciousness, are cases of telepathy within the individual brain. But however long the steps that thought may take in the rhythm pulsations by which it advances in brains of looser and coarsely woven tissue, it must now be always assumed to imply uninterrupted continuity of neural texture.

Even the fundamental theory of Bell has to be modified, so far as the brain is concerned, to meet the exigencies of the telepathic hypothesis. In Mr. Gurney's scheme of hallucination, centrifugal projection, or escape downward, may even be from the cortex through the basal ganglia to the peripheral organ. Qualified forms

of projection have been often assumed, but the matter is so complicated and so under dispute, that despite the strong light shed by Kandinsky, of whose chief and latest work Mr. Gurney has not taken account, we cannot see that the centrifugal theory of projection along sensory nerves can be proven, nor is needed. It involves the possible blocking of sensations in the *corona radiata*, does not take account of the fact that strong ideas do not usually excite hallucinations, and that as Galton has shown, great men are not prone to mental images. Impressions upon the senses may take on the psychic quality wherever they will in the passage inward to the cortex, one thing remains probable, viz.: that the more central the origin of impressions the more complex it is, and the more peripheral and sensuous the less attendant concepts there will be, the more the logical connection will be broken through and the less sense of inner activity there will be. More attention should have been bestowed to this point, with all the above tests and subjects. So far, however, as the phenomena are described or can be inferred, they indicate the same field of vision as real things, and suggest nothing akin to imperative ideas, projected sensations of central origin, rather than any subjectively created, or critically evolved sense of objectivity.

Very instructive is the experimental investigation of Mr. S. J. Davey on the errors of observation. He was some years ago nearly convinced of the truth of spiritualism by some sad and strange experiences, but was happily saved therefrom by learning and becoming very expert in a few tricks, especially that of slate writing. Assuming a professional name he gave seances to many intelligent people, requesting them to write down exactly what they saw. Many of these descriptions are published in a very long article in the proceedings for May, 1887. The sitters "never detected the *modus operandi*," and their conjectures about it are ingeniously diverse, and illustrate in many cases a strong propensity to a miraculous explanation. But the strangest thing of all is that we are not told how the trick was done, so that we have no point of departure from which to measure the amount of errors with each guesser. Whether it be that the love of mystification is stronger than the love of science with Mr. Davey, or whether he is under obligation of secrecy, which he does not even deem it necessary to state, his attitude is yet that of a conjuror pleased with his trick and the sense of human gullability it gives. A scientific man states the method by which he got his results; not so Mr. Davey. The society professes to desire to protect men from the common delusion in this field. In our judgment nothing whatever does this so effectively as explaining to them the method by which a few common effective illusions are produced. The acquisition of power to do these tricks it is easy to see was what saved Mr. Davey himself from the abyss of spiritualism, against which it is the most potent prophylactic. The trick-books do not retail the kind of illusion here involved, the conjuring business, if it is so desirable to save it, would not be injured materially. Spiritualists will persist, at least, till details are explained, that Mr. Davey is mistaken in thinking he used only natural means. It is almost impossible to exhaust the various methods by which some of the leading tricks are or may be done, but a good collection of descriptions of methods by which a few tricks most closely connected with the phenomena of spiritualism are done would, we think, in the end be the most effective of all disillusioning agencies.

Again, the theory of probabilities is perhaps the most instructive part of the modern logic of science, but the use made of it in these reports we regard as utterly misleading. Mr. Gurney even goes so far as to state that "the argument for thought transference cannot be expressed here in figures, as it requires 167 nines; that is, its probability is far more than the ninth power of a trillion to one." Has he forgotten the obvious truth stated by Mr. Edgeworth, in the first of his valuable papers, that the calculus of probabilities "is silent as to the nature of the agency, whether it is more likely to be vulgar illusion or extraordinary law." "This," he adds, "is a question to be decided not by formula of figures, but by general philosophy and common sense." M. Sorel well says that it is indispensable to consult experience frequently to know if the phenomenon can be sufficiently approximated to the ideal play of chance, as even games of so-called chance are not just applications of the theory of probabilities, though commonly thought to be. In the face of this commonplace we are obliged to say that the way in which appeal is so often made to this theory is the only thing in the work of the society which seems to us lacking in ingeniousness. This aside, however, there are other interesting incidents in these researches that shed light on the general applicability of this theory. Everything runs in groups. There is the Creery, the Guthrie, the Schmoll group, and, as Mr. Guthrie says, "the good averages run in lots," and he thinks that the calculus of probabilities does not help conviction, adding that one successful evening, when the conditions are good and the truthfulness of the percipient genuine and simple, is a better guarantee than any subsequent cross-examination of results. A friend of the writer missed in guessing the numbers of a die the first thirty-seven times, and if there had been as much interest in finding errors as successes, the former may have been as strangely grouped and bunched. As in games of chance, and in the records of gambling houses, there would seem to be as great individual marvels of bad as of good luck, did not the former always tend to be eliminated. In fact we have spent much time and labor in repeating with many subjects, nearly all the experiments of the English society, only to find in very many cases an unaccountable proportion of error. In many of these tests, at least, conditions are not known—not controlled—and the numerical basis needful for a fair average is not established, so that we do not know what "absolute chance" means in a given case, or what was the original *krasis* of things, what is the average error, or how errors are grouped. There is a sense, too, in which the probabilities against any given occurrence are infinite.

These points need fuller treatment, but we must hasten on to note the fallacious conception of evidence in such a field. Much is said about "spreading responsibility," the "cumulative" nature of the proof for telepathy, increasing the number of people who are knaves or idiots if it is not true, and the multiplication of instances is compared to increasing the size of a bundle of faggots, each one of which is easily broken, till together their evidential value is irresistible, and, last of all, prizes are offered for good tests, etc. In a word the society's conception of proof is quantitative. This is an imposing argument in America. When we are told that seven million children are following the Union Sunday-school lesson course, when enthusiastic spiritualists claim still more than that number of believers in their doctrines in this country as proof of pedagogic or

doctrinal merit, we reply that evidence is to be weighed, not measured by bulk. Quality of proof should be chiefly regarded in such matters as psychic research, and not quantity. If one-half the people in the world accepted telepathy and the other half rejected it, it would by no means follow that the probabilities pro and con were even. The cumulative method has the advantage of encouraging the bias above referred to both by mutual countenance and evidential appearances, but in science it is the competent minority that is usually right and the majority that is usually wrong. One man who would exhaust all the resources of modern science in precautions in this field, would be more authoritative than all the parlor seances together. What is to be chiefly desiderated is not the multiplication of instances, but more systematic and prolonged study of such cases as have been already found, the use of more cautions against error, the probability of which would be shown so incalculably great could the calculus be intelligently applied to their estimation.

Next to the fundamental assumption of telepathy in this class of cases we regard as the capital error of the society the association of the above so-called "experimental basis" with that class of phenomena illustrated by the seven hundred and two weird tales in the *Phantasms of the Living*, or with "spontaneous telepathy" at a distance. Mr. Gurney frankly admits here "a certain gap" or "incompleteness in our transition, which must be admitted without reserve," and yet elsewhere says it is impossible to tell whether he would have credited the validity of telepathy in the spontaneous phenomena had they not been confirmed by the "experimental basis." In the latter cases will and attention were involved to such an extent as to effect the robust health of Mr. Guthrie, and in the former cases consciousness is less involved. Mesmerism at a distance brings in other factors too abnormal to really constitute a transitional case. While spontaneous cases seem to occur at different distances, from a few feet to thousands of miles, no serious tests of the effect of time, or even distance, strange as it may seem, were made in the experimental cases. Mr. Creery thought that his daughters were most successful at the distance of a yard or two, and a few very inconclusive tests as to the effect of distance were made upon Mr. Smith, but there appears to be no reason to infer any experimental results save at very small distances, (if suggestion and trance is excluded), while for these distances the time seems to vary, with no suggestion or search for a cause, from an instant to fifteen minutes or more. To us the most natural and obvious inference, which is certainly not excluded, seems to be that the two series of cases are due to entirely different causes, no more related than are coincidence and collusion. Again the experimental results were chiefly in the field of the higher senses, involving special forms, and are matters of utter emotional indifference, while the spontaneous cases, which, indeed, sometimes touching the nadir of triviality, as in ghosts of clothes, warm water for shaving, etc., are mostly such as profoundly involve the affections, like death and danger of friends. The collection of tales is of the greatest value, and it is significant that it is the last moment of this life and not the first of another that seems to have most of Mr. Meyers' "telurgy." But we believe the final verdict of science respecting them will be that they illustrate the great mythopoetic tendency by which fancy unconsciously grows into similitude with fact, just as

organisms adapt themselves to their environment, a tendency that is rather to be sought below the threshold than "above the upper horizon of consciousness," as Mr. Meyers believes. The only psychological explanation we can suggest for the premature and almost passionate identification of the experimental and spontaneous cases as telepathic is the constraint of the potent but secret bias in favor of "superconscious" states, of a "soul-politic," or perhaps even "molecular meta-organisms," and in general toward "the solidarity of life, that realism proclaims." But this is surely the idealism of a Swedenborg, and not that of Plato or Hegel.

It illustrates, in contemporary form, Kant's "Dreams of a visionary explained by the dreams of a metaphysician." Our experience, in fact, is not unlike that of Kant, who, after paying a great price for the chief work of Swedenborg, and spending much time in its perusal, concluded, in substance, in his well-known article of the above title, that such pneumatological conceptions were pseudo ideas, formed by the negation of sense, made thought free from not *in* the world, and were idola illustrating the *morbid* tendency of some minds to come to a focus outside of themselves, and that for his part he would henceforth turn his back resolutely from the seductions of such considerations.

When we reflect how few are the well established facts that are exact and certain, and on the labor by which they were demonstrated, or on how rare are well ordered cohesions of thought or the associations that approach anything like real mental continuity, and on the inestimable educational value of these in making possible even a limited area of well woven mental tissue, and remember that modern science is already the greatest achievement of the human race, to bring one solid contribution, to which individuals are more and more content to spend a life of labor, we are reminded of Kant's well-known simile of an island surrounded by an unknown and very tempting, but foggy, stormy sea. In this sense telepathy is of the sea and not of the land. It is, on the whole, much less removed from modern spiritualism than from true science, so far as all telepathic theories go. Spiritualism, in its more vulgar form, is the sewerage of all the superstitions of the past. Wherever there has been civilization and culture, it is because its dark clouds have lifted for a space. It is the common enemy of science and true religion. It has led astray many able men. The beginning of science and philosophy has always been doubt of its claims. The majority of men, living and dead, are its adherents. It is against its claims that skepticism has its leading justification. To clear up its dismal jungles, and drain its unwholesome marshes, is probably the work of centuries. In modern biology, culminating in neurology, where so many of both the secrets and the revelations of science are coming to centre, that one might almost say the undevout neurologist is mad, a firm foothold is at length secured in which mind and matter, so long and so widely divorced that from the fallow between them wild and unsightly growths had waxed strong, and thick, and old, have a common interest, and the dangerous chasm between them is slowly closing. Physicians appeal to the imagination in desperate cases with bread pills and placebos, are less ashamed of interest in hypnotism and are less disposed to regard even hysteria as the *summum incognitum*, and the study of insanity as worthy of the briefest of all courses in medical schools, while clergymen and metaphysi-

cians, on the other hand, who used to practice healing arts in the good old time, when "Godlike was the doctor, who was also a philosopher," are beginning to take some interest in the body, and to read books on mind cures, and psycho-physics, hygiene and physiological psychology, and to realize that the student of religion and of idealism cannot, with impunity, neglect the study of the common forms of morbid psychosis. We desire, for our part, to see the psychological movement, which now seems destined to mark the present as the psychological, as the last quarter of a century has been the biologic-evolutionary age, kept in the severest sense, experimental and scientific. The dangers and difficulties are vast, and the specious false ways many, but we have a nucleus of solidly established facts, and the reward of every achievement is likely to be at least no less than any that have crowned the progress of science in the past. But we must ever remind ourselves that while "strange things are true, they are not truly known till they are related to what is tested, else they remain solitary and unfruitful."

Great credit is due the English society for calling attention afresh to the mysterious side of human life, and for later making known to English readers something of the valuable work of the French investigators of Paris, Nancy, etc. Mr. Meyers has taken great pains to see many of these men and their work. If good hypnotic subjects are more numerous in France than in England, it would seem that ghost seers are most common among cultivated classes in England. It is to be hoped, however, that the indication of more independent work in the study of abnormal states now apparent will lead to more solid results, and that the crude and premature theory of telepathy, which is by no means impossible, *per se* in some sense, but as yet lacks everything approaching proof save to amateurs and speculative psychologists will be allowed to lapse to forgetfulness. To the careful and patient experimenters and observers in this field there are now far better and far surer and far more useful results than these, though by methods far harder and slower. But it is by these that we prefer to labor.

Psychology. The Cognitive Powers. By JAMES MCCOSH, D. D., LL. D., etc., President of Princeton College. New York, 1886. Pp. 245.

Introduction to Psychological Theory. By BORDEN P. BOWNE, Professor of Philosophy in Boston University. New York, 1887. Pp. 329.

Psychology. By JOHN DEWEY, Ph. D., Assistant Professor of Philosophy in Michigan University. New York, 1887. Pp. 427.

The work first on the above list is to be supplemented by another on the motive powers of the mind, including conscience, emotions and will. The cognitive powers are here treated in three books as respectively presentative, representative and comparative. Dr. McCosh has taught psychology for thirty-four years, and compares his work to Uncle Toby's stockings, darned till hardly a thread of the original fabric remains. The book is neither dull or dry, but abounds in apt quotations in prose and poetry, stories, illustrations, sudden and unexpected but always impressive morals and hortatory passages, and seems to reflect, in the clearest and most direct way, the strong and beneficent personality of the author, not only